"It appears not unlikely that many of the so-called chemical elements may prove to be compounds of helium, or, in other words, that the helium atom is one of the secondary units with which the heavier atoms are built up. In this connection it is of interest to note that many of the elements differ in their atomic weight by four—the atomic weight of helium.

"If the α particle is a helium atom, at least three α particles must be expelled from uranium (238·5) to reduce its atomic weight to that of radium (225). It is known that five α particles are expelled from radium during its successive transformations. This would make the atomic weight of the final residue 225−20=205. This is very nearly the atomic weight of lead, 206·5. I have for some time considered it probable that lead is the end or final product of radium. The same suggestion has recently been made by Boltwood."

Then follows a discussion of the evidence on which this suggestion is based.

I think that the above quotation makes my position clear on this subject. E. RUTHERFORD.

McGill University, Montreal, October 11.

Radium and Geology.

THE Hon. R. J. Strutt has advanced weighty reasons in favour of supposing radium to be confined to a certain shallow layer over the surface of the earth. To assume, however, that a heavy element is thus restricted in distribution appears to me to present difficulties. It would appear that an *a priori* probable reason why uranium should disintegrate more rapidly near the surface than at greater depths would bridge over the difficulty, and, if for that reason only, would deserve attention.

I think such a reconciliation of observational facts with the probabilities involved would be found in the view that the break up of uranium is not entirely spontaneous, but is partly secondary in character, *i.e.* that disruption of an α particle from an unstable atom may precipitate the failure of neighbouring atoms, as Prof. J. J. Thomson has suggested might happen in the case of radium. If this be the case, and we assume that the uranium is in general distributed in random aggregates throughout the earth, a reason is at once forthcoming for Mr. Strutt's results. The lighter constituents in the outer crust—aluminium, silicon, oxygen—exert a lesser screening action than the heavy metals deeper down. The conflagration is, as it were, isolated where the heavier metals interpose to absorb the energy of the α ray which initiates the changes leading to radium. It is probable that if the absorption is adequate to reduce the kinetic energy below a certain critical amount, there would be no propagation of disruption.

The remarkable fact observed in Mr Strutt's experiments that radium is more abundant in the heavier silicates of plutonic rocks than in the lighter is not opposed to this view, but rather in keeping with it; and the absence of detectable radium in metallic meteorites need not be occasioned by the absence of uranium, but by the slower breakdown of the latter.

I cannot claim to speak authoritatively on the literature of this subject, but I can recall no other experiments bearing on this matter than those quoted by Prof. Rutherford in the last edition of his "Radio-activity." The case of uranium does not appear to have been investigated. Prof. Rutherford records an experiment in which he dissolved some pure radium bromide in 1000 times its bulk of a solution of barium chloride, and found no change in the γ radiation. I venture to suggest that this experiment is not conclusive. Increasing the volume 1000 times increases the average distance of the molecules but ten times, even were these fixed in the medium. This leaves the intervening distances still of the order of millionths of a centimetre. The heaviest metal brought to such tenuity would exert no appreciable screening influence, even from the α rays, to say nothing of more penetrating radiations. Mr. Eve's experiments, which are also quoted by Prof. Rutherford, are not, I think, to the point.

As cosmical effects of the greatest interest are involved, I think the question of how far radio-active effects are spontaneous deserves full investigation, and I think more especially with regard to the primary step, the generation of radium from uranium. If this is dependent on the matrix and on concentration, entirely new considerations arise.

It is not impossible, in the present meagre state of our knowledge, that the penetrating radiations observed at the surface of the earth have to do with the genesis of radium from uranium, the failure of such rays to penetrate deep into the crust limiting the production. The suggestion is continuous with that advanced above. J. JOLY.

Geological Laboratory, Trinity College, Dublin.

IN reply to Mr. O. Fisher's interesting letter of October 11 in this Journal under the above heading, it may be suggested that, though a state of stable thermal equilibrium exists now in the earth, it did not in the past, and that the earth has cooled down from a great initial temperature. We are, however, met with this difficulty, that the movements of the crust have been enormous in *late* geological times, as shown in the great mountain ranges of Tertiary date. This seems to be a fact entirely antagonistic to the suggested explanation.

No doubt some of the current geologico-dynamic theories will go to the wall should Mr. Strutt's interesting researches be confirmed, but I am of opinion that his work will ultimately prove helpful to sounder ideas of the origin of earth structure. T. MELLARD READE.

Park Corner, Blundellsands, October 13.

THE age of the great mountain ranges mentioned above by Mr. Reade, though comparatively late, is much earlier than that of the changes of vertical level investigated by Prof. Hull and Dr. Spencer to which I referred. They are evidenced by the drowned plains bordering the Atlantic on both sides, and by the deep cañons in them which are the continuations of existing river channels. These changes of level are considered to be of Pliocene or early Pleistocene date, and, therefore, geologically very recent. Godwin Austen came to a similar conclusion about the English Channel.

I thank Mr. Strutt for noticing (p. 610) my letter in NATURE of October 11. The fact of uranium not having been recorded in analyses of the rocks, as referred to by Mr. Strutt, has occurred to myself, but not being a chemist I have not alluded to it. But it seems to me that there ought to be an appreciable store of uranium present, large in proportion to the radium it is producing, if the latter is not permanent. That there is not appears to indicate that the disintegration of the radium, and therefore the escape of heat from it, is in some way checked in the earth's crust, as suggested by Mr. Rudge in his letter to the *Times* of August 18, and that consequently the temperature gradient is not due to radium in the crust, but to the cooling of the interior. I think it is in this direction that we must seek for a reconciliation between radium and geology.

Graveley, Huntingdon, October 19. O. FISHER.

Meteorological Data.

I SHALL be glad if you will enable me through your columns to make known to those interested in the collection of meteorological data the following information.

A number of copies of the Cape of Good Hope Magnetical and Meteorological Observations, vol. ii., "Meteorological Observations, 1841–6," have been placed at my disposal by the Controller of H.M. Stationery Office for distribution. The volume contains hourly observations, for each day, of pressure, temperature, and humidity, with a journal of other meteorological data.

I shall be glad if any scientific institution or library which desires a copy will be good enough to communicate with me upon the subject at the Meteorological Office, 63 Victoria Street.

I have also available for distribution in a similar manner a few copies of the following works:—

"Meteorological Observations taken during the Years 1829 to 1852, at the Ordnance Survey Office, Phœnix Park, Dublin, . . . and Other Places in Ireland."

"Abstracts from the Meteorological Observations taken at the Stations of the Royal Engineers (including 15 Colonial Stations) in the Year 1853–4, with Notes on Meteorological Subjects."

"Abstracts from the Meteorological Observations taken at the Stations of the Royal Engineers (comprising 13 British and 18 Colonial Stations) in the Years 1853–4, 1854–5, 1855–6, 1856–7, 1857–8, and 1858–9."

"Abstracts from the Meteorological Observations taken in the Years 1860–61, at the Royal Engineer Office, New Westminster, British Columbia."

These volumes will be issued without payment.

I may also mention at the same time that the Meteorological Committee, acting in accordance with the recommendation of the fourth International Conference on Scientific Aëronautics, has undertaken to subscribe for a number of copies of the international publication of the observations of the upper air on the "international days," which will be issued by Prof. Hergesell, the president of the commission. I shall be glad to know whether any scientific institution or library wishes to subscribe for a copy of this publication. The amount of the subscription is 1*l.* per annum. W. N. SHAW.

The Breeding Habits of the Tsetse-fly.

I SHOULD be greatly obliged if you could find space in your columns for the following extracts from a letter which I have received from my friend Dr. A. G. Bagshawe announcing the discovery, I believe for the first time, of the pupæ of the tsetse-fly (*Glossina palpalis*) in nature. As this species of fly is now known to be the agent which disseminates the infection of sleeping sickness, any discoveries relating to its breeding habits are of the utmost importance from the point of view of devising measures for extirpating the fly or checking its increase. Together with my colleagues Lieuts. Gray and Tulloch, I spent a great deal of time, when I was in Entebbe, in searching for the pupæ of the fly, and we offered the native boys a rupee each for them, but all our efforts to find them in nature were unsuccessful, although captive flies deposited great numbers of pupæ in our cages. I ought, perhaps, to explain at this point that the tsetse-fly is viviparous, and produces a full-grown larva, one at a time; the larva is of a light yellowish tint when born, and wriggles about actively for an hour or so, and then turns in a short time to a dark brown pupa, about the size of a grain of wheat.

Dr. Bagshawe, who is already well known for the botanical collections he has sent home, has succeeded where we failed, and as I do not know what steps he has taken to secure the priority for this most important discovery, I hasten to make it public on his behalf. It will be seen that the pupæ have been found in the banana plantations. Since bananas are the staple food of the Baganda, it would be impossible to destroy the plantations without creating a famine. I may mention, however, that we found the tsetse-fly swarming on the deserted island of Kimmi, on the Victoria Nyanza, where there were no plantations, so that this is perhaps not its only breeding place.

E. A. MINCHIN.

Lister Institute of Preventive Medicine, October 17.

(*Extract from Dr. Bagshawe's Letter.*)

"On August 29 I got them [the pupæ] at last. I had marked down a particular spot as likely, and had pitched my camp near by to search. Along the lake shore for about 100 yards was a belt of bananas 10–20 (40?) yards in width, and behind that undergrowth, going back 100 yards or more. Fly were thick and bothered one up to sunset.

"On the second day one of the porters I had coached brought me a pupa while I was searching a hole in a tree. He had found it among the banana rootlets. I searched there at once, and soon found some empty pupa cases. The next day I had a lot of my people at work and 53 pupæ were found, all in the loose crumbling soil round the bananas. In the scrub behind there are none to be got. . . .

"I made a series of experiments lately to find out how long a stretch of river the individual fly haunts. I started on the assumption that a fly with five legs is as good as one with six, and if one snipped off a piece of a known leg that fly could be identified when caught again. Six series of experiments could be made. It worked admirably. The experiments want repeating on a larger scale (I hope to do it on the Semliki), but I have shown clearly that the range is *at least* a mile. This is the reason why the breeding places have eluded search so long.

"(Signed) ARTHUR G. BAGSHAWE.

"Albert Edward Lake, September 1, 1906."

Suspended Germination of Seeds.

IN Mr. Claridge Druce's letter in NATURE of October 11 he rightly remarks that in order to prove the suspended germination of seeds over long periods, instances are required in which the factors of wind-carried seeds, &c., can be with some certainty eliminated. The following case, though not absolutely conclusive, may still be of interest.

Personally I am of opinion that the seed of Digitalis does preserve its germinating power for a considerable time. A few years ago I cleared a space, speaking from memory, of say forty yards by thirty yards, occupied by old Portugal laurels 25 feet to 30 feet high, planted fully sixty years ago, with *Rhod. ponticum* lining the path in front; the space, except on the path side, is surrounded by thick coverts. The nearest growing foxgloves were to the west along a 6-feet path running parallel with the long side of the cleared area, and distant, say, ten yards; both sides of this intervening space are lined by old rhododendrons; seed blown along would fall on the path or the edge of the clearing. The laurels were removed in January and February, when all, or nearly all, the seed would have been shed. Notwithstanding this, the next spring the whole of the cleared ground was covered with a uniform carpet of seedlings, practically hiding the bare ground. It seems to me that, even if some wind-blown seed penetrated the evergreen barrier, the seedlings would have appeared in patches.

I have known many other somewhat similar instances, but none quite so specialised as the above. I may add that the spot is exceptionally protected from wind, having tall forest trees on all sides.

ARCHIBALD BUCHAN-HEPBURN.

Smeaton-Hepburn, Prestonkirk.

Biometry and Biology.

OWING to the proof of my letter in last week's NATURE reaching me too late for careful revision, one or two slips escaped notice. Of these, I would wish to direct attention to the interchange of the words *intra-racial* and *inter-racial* in the second paragraph on p. 609 (column 1, line 14).

KARL PEARSON.

Biometric Laboratory, University College, London, October 19.

SPEED AND STABILITY IN RAILWAY TRAVELLING.

THE Salisbury railway accident, being followed after no very long period by the somewhat similar disaster at Grantham, undoubtedly raised a feeling of considerable uneasiness in the public mind. The recent publication by the Board of Trade of Major Pringle's report on the former calamity should do something to allay this apprehension, if only because it shows that the cause of the derailment of the train was not "mysterious," but is fully to be explained. That the evil we know is less alarming than one which vaguely threatens is a fact for which we have classic authority.

The accident occurred on July 1 at the Salisbury Station of the London and South-Western Railway, the train being the special boat express from Plymouth to London, carrying passengers who had arrived by the American liner *New York*. The train consisted of four eight-wheeled vehicles hauled by a four-